MEMORANDA

ON

ASIATIC CHOLERA,

ITS

MODE OF SPREADING,

AND ITS

PREVENTION.

BY

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"Obsta principiis."

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PREFACE TO SECOND EDITION.

I AM anxious to take the opportunity, which the publication of a Second Edition of this little pamphlet affords me, of saying an earnest word on the extreme importance to all places threatened by Cholera of establishing depôts where disinfectants may be had gratis or at cost price.

IN SUCH TIMES AS THESE NO HOUSE SHOULD BE WITHOUT ITS SUPPLY OF THESE PRECIOUS SAFEGUARDS.

Two well known facts will show at once what great issues may hang upon this.

The first is, that of the most malignant attacks of Cholera, a large proportion occur suddenly *in the dead of night*, when disinfectants if not at hand, cannot readily be got; and Second, That the discharges from a single case may, under conditions of no uncommon occurrence, become the means of spreading the disease through a large community.

If from this time, the practice were made general of throwing chloride of lime or some other disinfectant into privies and water-closets, every night and morning at least, in all places threatened by Cholera, the public safety would be immeasurably increased. In this way the care of the many might be made to counteract the negligence of the few. For the result of this practice would be that the sewers would be always charged with the antidote, and any choleraic poison that might chance to find its way into them would be at once bereft of its deadly power. In the way of a preventive measure nothing could be named which promises so much as this. There are some who even go so far as to believe that Cholera, although it might prevail with local severity, here and there, could scarcely become epidemic in the face of this precaution, provided the water supply were at the same time good. In comparison with the object to be obtained the expense would be slight, and would probably be more than repaid, by the improvement which would otherwise accrue in the public health.

Of the part which the public authorities, the managers of great public charities, and the great employers of labour, might take in

promoting this salutary measure I need not speak. A word to the wise is enough.

The paramount duty which devolves upon all Boards of Health of keeping the sewers of a district, in which Cholera prevails, thoroughly and constantly disinfected is well understood here, and under the guidance of our able Health Officer will, I have no doubt, be fully acted upon should Cholera come among us.

I may, however, repeat here a suggestion which I made in 1849, but which, as far as I know, has never yet been carried out. It is that in case of Cholera becoming rife, vessels provided with the means of evolving chlorine or sulphurous acid gas, should be suspended every night in the sewers of the infected districts.

In this way the sewers of a large neighbourhood might be disinfected at a small cost, through the dangerous hours of the night, and with the observance of the proper conditions as to level and so on, the disinfectant might be safely left to diffuse itself.

The Manor House, Clifton,
August, 1866.

INTRODUCTION.

"*Obsta principiis.*"

As Europe is once more invaded by Asiatic Cholera, I have thought it my duty to place the following brief Memoranda on the Propagation and Prevention of that pestilence in a separate form before the public.

The first in order, but not in date, are two Memoranda which were submitted by me in 1861, through the Inspector General of Her Majesty's Hospitals, to the Special Commission appointed to enquire into the Cholera Epidemic of 1861 in Northern India.

The method for preventing the spread of Cholera set forth in these Memoranda, was embodied by the Commissioners in the preventive code drawn up by them; and the Papers themselves were honoured with a prominent place in their Report.

The evidence adduced in these documents, is intended to support the theory that Asiatic Cholera is propagated exclusively by the characteristic discharges from the intestinal canal of persons affected with it; and further to show, that, under given conditions, the spread of the disease may be prevented by a plan which I was the first to suggest and employ, so long ago as 1849:—viz., by destroying by disinfectants, the specific powers of these discharges, immediately on their issue from the body.

The evidence which I have myself collected, in proof of these two propositions, has already in part appeared. I have much more in reserve, and should time permit I hope to place the whole before the public, at no distant period, in a complete and systematic form.

Meanwhile these brief notes will, perhaps, as being more quickly read and mastered, serve the present purpose better than a larger and more elaborate performance.

The need of new and more detailed evidence is the less pressing, because, under conditions which admit of being easily defined—such, for instance, as those which were found in the outbreak of Cholera at the Horfield Barracks, of which a narrative is sub-

joined—the method itself may be made a test of the truth of the theory.

Prevention and not theory is, moreover, our first care; and if we find the method successful, we can well afford to postpone for a time an examination of the various other data on which the theory is based. What is most wanted now, is a brief statement to show that facts already exist, which render it a duty to give the plan for preventing the spread of the Cholera, by disinfecting the characteristic discharges, a full and impartial trial. This object it is trusted the few short memoranda here strung together will fulfil.

It may be well to observe that even if the method in question be capable of all that may be anticipated of it, many conditions are needed which are essential to its success.

THE FIRST IS THAT IT BE APPLIED EARLY IN ANY GIVEN OUTBREAK.

THE SECOND, THAT IT BE EMPLOYED UNIVERSALLY.

AND THE THIRD, THAT ITS APPLICATION BE SUPERINTENDED BY VIGILANT AND FAITHFUL MEN, BACKED BY POWERS TO ENFORCE THEIR RECOMMENDATIONS.

Of these conditions, the first is obviously fundamental. It would be as rational to hope to exterminate mushrooms after they had been allowed to cast their spores to the wind, as it would be to put a stop to Cholera by this method, after the disease had already been suffered to gain much head.

Still, even under these circumstances, if the evidence already accumulated in its favour may be relied on, the effect of its employment must necessarily be to diminish the aggregate mortality.

In my own hands it has hitherto been uniformly followed by the results which theory would anticipate from it. How far it may be equally successful in the mixed and crowded population of large cities, and in various other conditions of life, uncivilized and other, actual and extensive trial of the method alone can determine.

Where the effects of a single lapse may be felt so widely in spreading the disease, it behoves us, perhaps, not to be too sanguine.

The poisonous germs of Cholera are so prolific, and their modes of distribution so various, and so hard to intercept, that such lapses are sure to occur.

Many difficulties in the way of the universal employment of the method are inherent in the present condition of the lower classes in this and other countries.

Those who have had most to do, practically, with efforts to stay epidemic disease, best know, how various and how great the impediments thus arising are.

To enumerate a few among them, only, I may mention, want—other sickness in the infected household—fatigue—sleep, in the exhausted attendants on the sick—stupidity—drink—recklessness—debauchery—wilful insubordination to advice or authority—and, lastly, a spirit of fatalism, which is much more common among the poor than is generally imagined. Then, again, comes the great class of vagrants, and of friendless men and women living in the low and common lodging-houses of large cities.

In seaports the condition of sailors—their abode in ships with no nurses at hand, and where cases may remain for some time undetected, and the loose and careless lives seafaring men so often lead, constitute a peculiar difficulty.

The occurrence of the disease in great public establishments, where the taint of a common latrine imparted by a single casual case, may communicate the disease at once to large numbers of persons, is another special danger.

This is a contingency which has, by no means, been taken sufficiently into account in studying the course of events in city outbreaks.

A very striking illustration of it appears to have occurred in the last great Cholera Epidemic at Munich.

It seems to have been ascertained that the germ of the disease was brought to Munich by Italians, who had come there in charge of goods for exhibition in the Munich Crystal Palace.

Many of these Italians, coming from places where Cholera was reigning, were seized with Choleraic diarrhœa, or in other words, with Cholera in a light form, and the latrines of the Crystal Palace were tainted by them.

The overlookers of the Palace, resorting to these latrines, received the infection in great numbers, and, as scarcely more than little lots of two and three lived in the same street, Cholera broke out in a short time, almost simultaneously, in nearly every part of the city.*

Another, and an equally special difficulty, arises from the occurrence where Cholera is epidemic, of numerous cases of Diarrhœa, which are, in reality, cases of Cholera in a mild form, but which not being recognised as such, are not so dealt with. The gravity of this circumstances arises from the fact, that these cases are often the source of germs which communicate the disease to other persons in the most malignant degree.

Many of the adverse conditions here specified, may, however, be in great part overcome by a powerful central authority, supported by an active and energetic staff, and a fitting organisation.

Whatever the practical success of the method here proposed may be, I am convinced, not only from long thought on the

*See "Pettenkofer's Verbreitungsart der Cholera," p. 68 and Seq.

subject, but from extensive personal trial of the method itself, that in it we shall find the best protection against this fatal pest. Two negative recommendations, at least it has; first: it can do no harm, and, secondly, every other method hitherto tried for the prevention of Cholera has signally failed.

If successful one most happy result this method will immediately have:—that of entirely preventing the terror and moral disorganisation on the approach of Cholera, of which such painful accounts have lately reached us from other countries.

It has often been said, that it is wrong to teach that Cholera is contagious, on account of the panic, and neglect of the sick to which the doctrine leads.

But if we can show, at the same time, that the contagion may be disarmed by such simple measures as those here suggested, all panic is at an end.

I have appended to the reprints a short outline of a practical scheme for the prevention of Cholera.

In doing this I need scarcely say that I am only putting into a condensed form my own ideas on the subject of Cholera, and am actuated by no desire to impose on others views which they may not be prepared to endorse.

The Manor House, Clifton,
August, 1865.

MEMORANDUM

ON

THE PROPAGATION AND PREVENTION

OF

ASIATIC CHOLERA.

By WILLIAM BUDD, M.D.

EDINBURGH,

SENIOR PHYSICIAN TO THE BRISTOL ROYAL INFIRMARY.

(Reprinted from the Report of the Special Commission appointed to enquire into the Cholera Epidemic of 1861 *in Northern India.)*

The conclusions to which I have been led, regarding the spread of Asiatic Cholera, may be stated in few words. In their most general expression they are summed up in the two following propositions:

1.—That the disease is essentially contagious or communicable; and

2.—That it is disseminated (as I believe exclusively) by the liquid discharges from the intestinal canal of Cholera patients.

To understand clearly the power of a disorder, spreading in the way here assumed, to affect such large numbers of persons as those who suffer in a Cholera epidemic, it is necessary to bear in mind the one great characteristic of the group of contagious poisons which give rise to acute diseases. I speak of the enormous multiplication which these poisons undergo in the living body, as the result of the morbid process they set up. It is, in fact, this multiplication, and the disturbances attaching to it, that, in each case constitutes the disease and destroys life.

In Small-pox the work of reproduction is seen in results directly appreciable to the eye. The practice of inoculation has shown, by numberless instances, that under certain conditions a perfectly impalpable speck of the Small-pox virus inserted into the skin may produce a disease, which, in the course of a few

days, issues in the development of a new stock of the same virus, sufficient in amount to inoculate with Small-pox myriads of other persons.

Although the fact is not open to evidence of the same precise order, yet I believe it to be equally sure, that in every case of Asiatic Cholera also, the Choleraic poison is multiplied in a ratio at least as great.

Cast off by the intestine of the Cholera patient, in the characteristic rice-water discharge, this poison may be transmitted to other and uninfected persons, in the following principal ways:—

1.—By the soiled hands of attendants on the sick; a mode of communication which is probably very common within the limits of the family circle.

2.—By means of bed and body linen, and other articles, tainted with the rice-water discharges; and

3.—Through the medium of the soil.

As the discharges are liquid, the great bulk of them necessarily finds its way to the ground. Thus deposited, the poison may take effect: (*a*) by rising into the air with the products of evaporation; (*b*) by percolating into the drinking water; and lastly, by atmospheric dispersion, in the form of impalpable dust, after it has passed into the dried state.

That the disease may be communicated by poison in this last form, is, perhaps, difficult to establish by direct proof. But in default of this, the case is one in which the evidence furnished by analogy is so close and weighty, as to leave no reasonable doubt of the fact.

We know, *by experiment*, that other contagious poisons when dried (the vaccine, the variolous, and the woorara poison, for example) retain their properties in a dormant state for indefinite periods of time, recovering them entirely when moistened. We know, by evidence almost as certain as that of experiment, that the same is true of the poison of scarlet fever, of that of malignant pustule, &c., &c.

It would be contrary to all probability that it should fare otherwise with the Cholera poison; and in fact, in the numerous and well authenticated instances of infection conveyed by articles of clothing, this poison, during its transport, must necessarily have been, in many, in the dried state.

As this state is one which entirely protects organic bodies from molecular change, there would seem to be no definite limit to the time during which the morbific agent, having once passed into it, might retain its specific powers.

If the foregoing premises be admitted, certain consequences may at once be deduced from them, which it is all-important to note.

From the enormous multiplication which the poison undergoes in the living body on the one hand, and from the nature of its

various modes of far-reaching distribution on the other, two things must necessarily follow. The first is, that under certain conditions, and conditions that must really often happen, a single case may give rise to a wide spread infection; and, secondly, that, as cases multiply, it must in numberless instances be as impossible to trace their linear succession, as it would be to trace that of particular generations of fungi or infusoria. In both examples, in fact,—in the disease as well as in the living organism—the organic germs are disseminated, not only through the same media, but in the same modes and by the same agencies. The necessary result is, that in both, new specimens must be constantly springing up whose direct parentage it is impossible to trace, and under circumstances which, to those who have not the key to the events, bear all the semblance of spontaneous origin.

When matters have come to this pass, it is obvious that the diffusion of the disease will, for the most part, occur in an order entirely different from that to which the idea of contagion is commonly attached.

To apprehend clearly the course of events in a Cholera epidemic, another element must be taken into account, namely, the extreme shortness of the period of incubation. In most other acute contagions, the average duration of this period is a fortnight or more; in Cholera, it seldom exceeds three days, and where the disease is virulent, there is evidence to show that it may not exceed twice that number of hours. Hence the proportionately greater rapidity with which Cholera extends itself through an infected community.

The relative share which the various modes of dissemination here indicated take in the propagation of the pestilence, must vary with season and climate, with temperature, with the habits of the people, the nature of the soil, with that of the water supply, and other sanitary arrangements.

From what I can learn of Indian habits, and of the arrangements which prevail in India in regard to drinking water, it seems probable that, in that country, water communication plays a very important part in it. In particular, there seems to be reason to believe that the use of water impregnated with Choleraic germs is answerable for the sudden outbreaks of the disease which have sometimes been observed in bodies of troops on the march, and in which large numbers of men have been struck down almost at once.

On the other hand, from the rapidity with which the rice-water discharges must often pass into a dry state, under the burning rays of a tropical sun, it seems highly probable also that, in India, "Cholera dust," if I may use such an expression, has a large share in the work of propagation.

As in most places where the disease prevails some germs must

almost necessarily remain in a state of desiccation, it is easy to see how every epidemic may leave behind it in a dormant form the seeds of a new outbreak, co-extensive with the first.

The evidence which has led me to these various conclusions has been in part already published in a series of letters addressed to the Association Journal in 1854-55. Of these letters, I have the honor to forward an abstract, in the form of a communication to the Edinburgh Medical Journal, by the late Professor Alison, "On the Communicability of Cholera by dejections."* Since that date I have collected a great body of additional evidence to the same effect, which I hope in the course of time to make public. Meanwhile, it may be readily shown that the foregoing views explain, in the most natural way, almost all the leading facts which characterize the diffusion of this pestilence.

They explain, in the first place, its relation to filthy habits and defective drainage; its predominance in low levels; its striking tendency to follow the natural line of watershed; and its communication to persons, who not only have never been in the presence of the sick, but who are stationed at a distance from them.

If these views be just, the disease may be described in general terms, as "a disease which infects the ground." It is impossible to look at any of the maps which have been constructed to represent particular Cholera outbreaks, without at once recognising this characteristic. In all such maps, in addition to the "dots" which mark the occurrence of cases in single order, there are others which lie in such thick clusters as to look at a distance like a stain on the paper. These clustered dots always occur where drainage is either defective or absent, and the stain on the map corresponds to, and is the expression of, a miasmatic stain of the soil by the liquid poison.

In addition to these, some other circumstances of a very special kind have been observed in particular outbreaks, in which the concidence between theory and fact is still more striking. Among these, perhaps the most remarkable relate to cases of public establishments for the reception of large numbers of both sexes, and in which the disease, in a particular outbreak, has been entirely confined to one sex. Many instances have happened in which, of large bodies of men and women, living under the same roof, eating the same food, drinking the same water, and breathing the same air,—sometimes, indeed, with only a partition-wall between them,—one sex has been decimated by Cholera, while the other has remained entirely exempt.

The whole of this strange result may be accounted for by the fact that, in almost all such establishments, there is one common

* "Edinburgh Medical Journal," December, 1855.

privy for either sex, which is devoted to its exclusive and separate use. The contamination of this privy, by rice-water discharges of the first casual case, explains at once the whole mystery.

The force of this example is the greater, because, under any other possible view, the fact appears to be entirely inexplicable.

The operation of tainted privies, in disseminating the disease in work-houses, prisons, barracks, and other public establishments, is pointed out in one of the extracts which Professor Alison has given from my letter, in terms to which I have nothing to add.

The theory, that Cholera is disseminated by the dejections, seems also to be remarkably confirmed by the way in which its prevalence is related to rain-fall.

In Europe, all the great epidemics have occurred, as we should have expected them to do, in times of prolonged drought.

Rain by diluting the poison, and by giving rise to floods which rapidly sweep it beyond the inhabited area, seems to have a powerful influence in checking the disease. But to have this effect, the rain-fall must be heavy and continuous. There are many conditions, under which it is readily conceivable that light and intermittent rains may, on the contrary, favour its spread.

I have not yet completed my study of the history of Cholera in India, but I believe I am right in assuming that there also the seasons of great epidemic have generally been dry seasons.*

Whether the cessation of the recent epidemic, on the occurrence of the great floods which immediately succeeded to it, be more than coincidence, the details given by the papers do not enable one to judge.

Before concluding this part of the subject, there is one other peculiarity of the cholera poison, on which I desire to say a few words, as its recognition is essential to a clear understanding of some of the anomalies attaching to Cholera outbreaks, which have most perplexed Medical observers. I speak of the tendency of the poison, when in a moist state, to rapid, spontaneous decomposition.

In one of my letters to the Association Journal, in 1854, after

* It need scarcely be said that the influence of these external conditions, important though it be, is secondary only. Acting in various complication, many of them in direct antagonism, and each subject to great fluctuations, the exact effect of any one of these conditions can never be predicated with certainty. Thus, although, drought has appeared greatly to favour the spread of Cholera in Europe, the same cannot be said of all parts of the globe. From the Report of the Calcutta Cholera Commission, it appears that while the mortality from Cholera in the Eastern part of India is highest as a rule, in dry seasons, precisely the contrary holds of the West. Possibly this apparent contradiction may be explained by some difference in the arrangements for the supply of drinking water. It is quite possible, also, from the difference in the rate of evaporation that the effect of rain may not be the same in a tropical as in a temperate climate.

entering into some speculations as to the actual nature of the morbific agent, I expressed myself in the following words :—

"But if we are still in ignorance of the actual nature of the agent, "there is one of its distinguishing properties about which there can be "no mistake. I speak of its natural tendency, except under special "conditions, to rapid decay and extinction. I have already drawn "attention to the extraordinary rapidity of its development,—a rapidity "unexampled in the case of any other contagious poison. Its tendency "to decay appears to be rapid and unexampled in the same proportion. "This is no more than we should have expected. Through the whole "of organic nature, rapid growth and rapid decay are, of necessity, "correlative terms.

"What we should have concluded, as to this property of the Choleraic "poison, as a matter of inference, is seen as a matter of fact on every "hand, wherever Cholera breaks out. Its consequences are exemplified "in a thousand forms: broadly, in the short duration and rapid subsi- "dence of particular epidemics; and less broadly, but not less plainly, "in the rapid disappearance of the disease in Courts and other localities "where it had lately raged, and where a large prey had seemed still to "invite its deadly attack; yet more especially in the rapid, spontaneous "disinfection of infected linen, of which many striking instances might "be recorded, to say nothing of numberless other facts to the same "purport.

"It seems to be certain, in fact, that where the stock of poison is not "renewed by renewed development in the bodies of living men, it cannot "long subsist in the common conditions of the English climate. This is, "no doubt, the reason why, although Cholera has made great havoc "among us in certain favourable seasons, it has failed to establish itself "permanently here. In India—the native home of the disorder—the "atmospheric conditions appear to be more favourable to the preserva- "tion of the powers of the poison; partly, most probably, from the "higher temperature of the climate; partly too, from the operation of "circumstances, which must often cause the poison to pass rapidly, im- "mediately on its elimination from the human body, into the dried state.

"It is from not having recognised its great destructibility, and rapid "tendency to decay, as the special and distinctive character of the "Choleraic poison, that physicians have been led to draw inferences "from facts such as those just referred to, which the facts themselves "by no means warrant."

These considerations (although they might no doubt, have been better expressed,) I believe to be perfectly just.

The powers of all the contagious poisons appear to be due to their being in an active state of metamorphosis, and to be proportioned to its intensity in each particular case. Where the conditions necessary to re-production are present, a rapid multiplication of the poison is the result. Where they are absent, an equally rapid destruction of the original stock.

The cells of yeast, when placed in a simple saccharine solution, enter into fermentation, and speedily perish. But if the same cells be placed in a similar solution, but one which contains as well the nitrogenous elements for their growth, the very same metamorphosis which led, in the former case, to their destruction, leads now to the development of a new brood.

Under this view, as applied to Cholera, the shortness of incubation, the rapid course of the morbid phenomena, and the rapid subsidence of the disease often observed in infected localities, are circumstances which have their root in a single condition, and are all mutually related.

By reference to them, many of the peculiarities which distinguish epidemics of Cholera from those of other acute contagions may be readily explained, which would otherwise remain quite unintelligible.

I do not know whether I am warranted in assuming that the case, of which an outline has here been briefly sketched, is already a strong one on its own grounds. But in addition to these, it has the support of analogy of no common force.

In every country in Europe, the almost perfect identity of the conditions which promote the spread of Cholera, and of those which are known to promote the spread of Typhoid Fever, has been made the theme of general remark. In the Cholera Report of the London Royal College of Physicians, this identity, as regards one great class of conditions, is made the subject of a striking and elaborate parallel. In every successive epidemic of Cholera which has visited England, the burden of the teaching of the English Board of Health has been, that Cholera and Typhoid Fever are the offspring of precisely the same sanitary defects.

Having this in view, I have ventured to enclose a series of papers by myself, on the propagation of Typhoid Fever, containing evidence to show that the defects in question have the effect ascribed to them, only because this Fever is (mainly) disseminated by the discharges from the diseased intestines of already infected persons.* In other words, the striking family-likeness between the two diseases, as regards their mode of spreading, is due to the fact that Typhoid Fever also is "one of the great group of diseases which infect the ground."

It is, I need scarcely add, impossible, within the brief limits of a memorandum, to discuss the various objections which may be taken to the foregoing views. I may, however, state that, having in the course of many years' study of the problem, given my mind much to this aspect of it, I have hitherto met with no objection which would not apply with still greater force to any other mode of accounting for the facts. I may further, perhaps, be allowed to take this opportunity of observing that where a theory rests on evidence of its own, facts that seem to be opposed to it, although they may possibly require some qualification of it, constitute no valid plea for its rejection.

It is well known, for instance,—to illustrate small things by great ones—that the theory of universal gravitation would never

* These Papers will be found in the *Lancet* for 1859-60, and in the *British Medical Journal* for 1861.

have been established, if the minor difficulties which appeared to stand in its way at the date of its promulgation had been allowed to prevail against it. The same thing is still more true of the undulatory theory of light. In this last case, indeed, there remain, even now, one or two outlying phenomena, which no one has yet been able to bring within the domain of the theory. That light is propagated by the undulations of an ether is not the less the scientific faith of Europe.

The preventive measures I would venture to suggest for checking the spread of Cholera are directly founded on the conclusions to which I have been led regarding its propagation.

Their object would be to destroy, by means of chemical disinfectants, the poisonous properties of the discharges, immediately on their issue from the body; and in places where this has been neglected, to remove, if possible, all uninfected persons from the poisoned ground.

The particular methods by which these ends may be accomplished are described in sufficient detail in the paper from the Edinburgh Medical Journal; and a memorandum in M.S.* (also enclosed), which I drew up some years ago, at the request of a friend, for arresting the spread of Cholera among troops in infected Barracks.

An important practical difficulty in the way of complete disinfection arises from the circumstance that in the last stages of Cholera, the discharges are almost always involuntary. This difficulty may, however, be in great measure got over by placing, under the breech of the Cholera patient, a bag, containing a sufficiency of Condy's or Mc Dougall's disinfecting powder,—to be changed as often as occasion may require.

In the absence of special disinfectants, a good deal may yet be done by one or two simple measures for limiting the sphere of the operation of the poison.

It is an established fact that the noxious powers of animal poisons are destroyed by the temperature of boiling water. In boiling water we possess, therefore, a ready means for the disinfection of soiled linen, bedding, and other tainted articles.

By digging a trench in the ground, in a well-chosen spot, for the reception of the rice-water discharges, and by covering each discharge as it is deposited by a layer of earth, these dangerous morbid products may be disposed of in a way that, at any rate, would prevent their having a wide-spread effect.

It may be well to add that these various measures, which are devised for the emergencies of actual disease, are in nowise intended to supersede the necessity of general sanitary improvements.

This Memorandum will be found at page 18.

In all large public establishments there is one thing which should be made an essential part of their internal economy. I refer to the establishment of a separate cloaca, for the reception of all discharges from sick persons. If the corresponding water-closet or latrine were provided with self-acting means of disinfection, the security would be all the greater.

The contagious diseases which are communicated by exuviæ from the intestine, are so frequent and so rapid in their course as to leave no doubt that an arrangement of this kind would protect the inmates of such establishments from many a sudden and unforeseen outbreak.

I cannot conclude this memorandum without drawing attention to the very important works on the same subject by the late Dr. Snow.

I was led to the principal opinions I hold, respecting the propagation of Cholera, before the appearance of his first Memoir, and on entirely independent grounds. I had also taught, for many years, at the Bristol Medical School, the leading doctrines regarding the propagation of Typhoid Fever, which are enumerated in the series of papers herewith enclosed. But Dr. Snow was the first to announce publicly that Asiatic Cholera is disseminated by the rice-water discharges, and to substantiate the statement by evidence which, for my own part, I consider to be perfectly conclusive.

Clifton, Bristol,
4th November, 1861.

PLAN

(Reprinted from the Calcutta Report)

FOR

ARRESTING CHOLERA AMONG TROOPS IN INFECTED BARRACKS.

1st.—The troops to be removed at once, if possible, foom the infected Barracks, and to be encamped on high ground.

2nd.—All who may be attacked after their removal, to be separated from their healthy comrades the moment the first symptoms appear.

3rd.—All discharges from the sick (including those vomited) to be received, on their issue from the body, into vessels containing a saturated solution of chloride of zinc, or some other powerful disinfectant.

4th.—The latrines or other places which serve as the final receptacles of these discharges, *to be reserved exclusively for that use.*

5th.—All latrines belonging to the Camp to be strewed, night and morning, with a mixture of peat charcoal and chloride of lime.

6th.—All tainted linen, bedding, &c., to be burnt immediately after its removal from the sick.

7th.—The hands of all attendants on the sick to be scrupulously cleaned (by means of sand and water, containing some disinfectant), whenever they have became soiled by "rice-water."

8th.—The utmost care to be taken in providing drinking water; and, where any doubt exists as to its quality, the water to be boiled and filtered through charcoal before use.

9th.—The interior of the infected Barracks to be fumigated with chlorine or sulphurous acid.

10th.—Existing privies and the drains connected with them to be built over, or otherwise abolished where necessary, after a preliminary disinfection by the free use of chloride of lime or peat charcoal.

11th.—Convalescents to be kept, for some time, in separation from the healthy troops.

CLIFTON, *November*, 1861.